ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *June* 9, 1854. No. 8.

G. B. AIRY, Esq. President, in the Chair.

James Samuel, Esq., 26 Great George Street, Westminster;
Lieut. Andrew Noble, Royal Artillery;
Lieut. Wm. Hett Noble, Royal Engineers; and
James Burman, Esq., Castle Rushin, Isle of Man,

were balloted for and duly elected Fellows of the Society.

Dr. Luther, of Bilk,

was balloted for and duly elected an Associate of the Society.

The Astronomer Royal has the pleasure to communicate to British astronomers the following letter, which he has received from the Secretary of the Post-Office. The Astronomer Royal wishes at the same time to direct the notice of astronomers to the liberality and obliging attention of the Secretary of the Post-Office, who, without further application from himself, has spontaneously adverted to the practices in the introduction of the *Astronomische Nachrichten* to this country.*

"*General Post-Office, 14th June*, 1854.

"Sir,—Referring to the recent correspondence upon the subject of the charges which had been made in this country upon certain copies of the *Astronomische Nachrichten*, transmitted through the Post from Hamburgh, I beg leave to inform you that the Director of the Posts at Hamburgh has forwarded to this office some copies of the publication in question, with their covers, from which it appears, that through some inadvertency these copies, and possibly other copies, have been charged with postage in this office, although it is well known to the officers, through whose hands these publications usually pass, that they are entitled to be delivered without charge.

* The *Astronomische Nachrichten* is now regularly forwarded to this country free of postage, and the blank occasioned by the non-transmission of several numbers is being gradually filled up.—EDITOR.

"I very much regret these mistakes which have occasioned so much annoyance, and I have given stringent instructions to the various officers concerned to prevent by all means any future errors of the same kind.

"I have also communicated with the Director of the Posts at Hamburgh to the same effect.—I am, Sir, your obedient, humble Servant,

"J. TILLEY,
"Assistant-Secretary.

"*Professor Airy,*
"*Royal Observatory, Greenwich.*"

Discovery of a New Comet (*Comet III.* 1854.)

(*Letter from Professor Argelander to the Astronomer Royal.*)

"On the 4th of June Herr Klinkerfues, in Göttingen, discovered a new comet. As I have just learned that the discoverer has not himself made this appearance known to you, I do myself the honour of informing you; and I add at the same time the positions obtained by the discoverer himself, as well as the only ones which the weather has until now allowed us to make. The comet is very bright, and could be seen very well in the finder the day before yesterday, very near to the horizon, notwithstanding the light of the full moon. It has a remarkable thickening near the middle, but neither a defined nucleus nor a tail.

"The places are derived only from rough calculation:—

Date.			R.A.	Decl.
	h m s		h m s	° ′ ″
June 4	14 0 0	Göttingen	2 1 15	+ 32 15 —
5	13 37 27	—	2 5 23·25	+ 33 40 18·6
	13 45 19	—	2 5 24·22	+ 33 40 43·9
			° ′	° ′ ″
11	12 2 48·0	Bonn	40 47·79	+ 43 56 44·7
	12 33 30·4	—	40 58·07	+ 43 59 13·0

Bonn, June 13, 1854.

The following elements of this comet have been calculated from the Göttingen Observations of June 5, and the Berlin Observations of June 9 and 17, by M. Klinkerfues:—

T	.....	June 22·12174
π	.....	° ′ ″ 272 58 5·5 } Equinox,
Ω	.....	347 48 44·9 } 1854·0
i	.....	71 8 21·0
Log q	.....	9·811244

Motion retrograde.

These elements have some resemblance to those of the comets of 961 and 1558.

Observations of Comet III. 1854, *at Mr. Bishop's Observatory, Regent's Park.*

June 28, at $11^h\ 36^m\ 49^s$ G.M.T.; R.A. 110° 42′ 26″; Decl. + 59° 42′ 58″.

The following calculated places of the comet have been furnished by Mr. Hind:—

0h Gr. M.T.	α	δ	Δ	r
	° ′	° ′		
July 16	150 52	+39 0	1·406	0·818
26	157 36	29 31	1·668	0·951
Aug. 5	161 39	22 22	1·922	1·097
15	164 17	+16 8	2·330	1·247

An Account of the Observatory at Hartwell House.
By John Lee, Esq., LL.D., F.R.S., &c.

"In presenting an account of my observatory to the Astronomical Society, it is not my intention to enter into any lengthened detail, as both the building itself and the principal instruments which it contains are minutely described by my excellent friend Admiral Smyth in the *Ædes Hartwellianæ* and in the *Celestial Cycle*. The former work, which was printed for private circulation only, contains an account of my first small observatory with its fixed and portable instruments. Here the tangible advantages of practical astronomy followed the physical theory, which formed a prominent part of my college reading; and the pleasures I have thus experienced, of all others perhaps the purest we are permitted to enjoy, have induced me to associate with myself as astronomical fellow-labourers my friends and neighbours at Aylesbury, Hartwell, and Stone. The three observatories which have recently been erected in these localities have already been described at the meetings of this Society; and to complete the cycle, I wish to offer the following short description of my own.

"My second and larger observatory adjoining the south-east angle of Hartwell House, consists of an ante-room, transit-room, and equatoreal tower, in the erection of which no expense was spared in order to insure the approbation of the most critical astronomer. Admiral Smyth, the Chairman of our synod, availed himself of the advice and labours of the elder Tully, Mr. Dollond, Mr. Vulliamy, Mr. C. May, and Mr. Thomas Jones, and, therefore, in the construction of both the building and the instruments he was determined to be satisfied with nothing short of the utmost attainable perfection. That the work of the practical optician and engineer was sufficiently successful, is evident from the meed of praise which it has received in the *Ædes Hartwellianæ*, as well as from the fact especially recorded by the Admiral himself, that he was generally enabled to give to his Hartwell measures of double stars *high weights*.

"The model now exhibited of the observatory, by our village carpenter, Mr. G. Carter, is on the scale of an inch to the foot, and

points out in detail the position and size of the instruments. The ante-room is 11 feet by $8\frac{1}{2}$ feet. The transit-room is 16 feet 5 inches by 11 feet 2 inches, and 10 feet 5 inches in height; the tower $15\frac{1}{4}$ feet in diameter, and 16 feet in height. The north pier in the tower is a single block of Portland stone, and is 12 feet 3 inches high and 3 feet square at the base. The polar axis, which is 10 inches square in the centre and 8 inches at the two extremities, is 13 feet long. The piers are built upon a bed of concrete, which is placed upon the limestone rock at a depth of seven feet below the surface. The walls are 14 inches thick and covered with a thick coat of cement.

"The transit instrument 5 feet focal length, with an object-glass $3\frac{1}{4}$ inches in diameter, was supplied by Mr. T. Jones, of Charing Cross, at a cost of 105*l*. It is supported by two piers of Bath stone, and borne at the centre by two well-proportioned cones, which terminate in rigorously turned pivots of bell-metal of $1\frac{1}{4}$ inch in diameter. These pivots, which are 30 inches apart, rest upon Y's, also of bell-metal, but the latter are additionally hardened by the introduction of a thin slice of Brazil pebble. The adjustment of the transit instrument is tested by two meridian marks, which are placed respectively 100 feet north and south of the observatory slit, and are inserted into blocks of marble—that on the north side representing the Temple of Janus as given on a large brass medal of Nero, and that on the south side being a miniature of the façade of the Temple of Concord at Girgenti.

"The excellent clock by Vulliamy, with Graham's dead-beat escapement, cost also 105*l*.

"An object-glass for the equatoreal was purchased from Sir James South for 200*l*., but on the dismantling of the Bedford Observatory, Tulley's *chef-d'œuvre*, 5·9 inches in diameter and 10 feet focal length, which had been purchased by Admiral Smyth from Sir James South for 220*l*., was transferred to Hartwell with its equatoreal mounting by Dollond, and its equatoreal clock invented by the Rev. R. Sheepshanks, and re-made in its essential parts by Mr. C. May. This fine instrument has been duly described by Admiral Smyth in his *Cycle of Celestial Objects*, and in the *Memoirs* of the Society.

"It would be difficult from lapse of time to calculate the expenses of the whole Observatory, and, perhaps, I should feel unwilling to look at its amount. But I may say for the encouragement of younger amateurs, that many a sportsman expends more on his 'Joe Manton' and his dogs, in order to observe the altitude and azimuth of a brace of birds, than a practical astronomer *need lay out* in a most efficient battery for securing the position and distance of double stars.

"The niches in the Observatory are ornamented with busts of Newton, Laplace, and Mrs. Somerville, and portraits of eminent astronomers adorn its walls.

"I may add that meteorological observations are made daily with a complete set of instruments, which were approved by James

Glaisher, Esq., the indefatigable Secretary of the British Meteorological Society, and the monthly returns are sent to the Registrar General."

Dr. Lee appended to the foregoing account *A Genealogical Table of the Descendants of the Bedford Observatory.* It appears from this Table that the Bedford Observatory was erected in the year 1827; and that it has been followed in direct succession by the construction of ten or eleven other observatories throughout the country.

Mean Places, as determined by Meridian Observations at the Cambridge Observatory, of the Stars of Comparison used by Mr. Maclear at the Cape of Good Hope in Observations of Comet II. 1853. (*Schweizer's Comet.*)

See Monthly Notices, Vol. xiv., No. I., p. 8.

Ref. No.	Name of Star.	Mean R.A. of Star Jan. 1, 1854. h m s	No. of Transit Obs.	Mean N.P.D. of ✻ Jan. 1, 1854. ° ′ ″	No. of Circle Obs.
1		4 59 52·32	2	103 19 20·63	3
2	H.C. 11551	5 57 33·44	2	104 4 57·36	5
3		5 57 55·39	3	104 1 38·67	3
4		5 58 14·21	2	104 6 49·58	3
5		6 11 56·91	3	104 18 19·60	3
6		6 12 48·78	2	104 16 43·45	3
7		6 14 35·97	3	104 14 27·82	4
8	H.C. 12825	6 32 36·39	3	104 1 5·18	4
9	B.A.C. 2221	6 40 11·56	3	104 16 22·08	3
10		6 42 11·88	3	104 13 44·49	3
11		6 47 39·40	3	104 3 48·17	3
12	H.C. 13392	6 48 6·38	3	103 58 24·34	2
13	μ Canis Majoris	6 49 25·22	3	103 51 28·46	3
14		6 56 23·24	2	103 57 31·47	3
15		6 58 19·26	3	103 45 4·95	3
16	H.C. 13848	7 0 41·58	3	103 49 38·05	3
17		7 1 53·54	3	103 45 37·84	3
18		7 8 41·26	3	103 32 54·11	4
19		7 10 3·02	2	103 25 28·99	3
20	H.C. 14228	7 11 14·87	3	103 37 47·73	2
21		7 11 19·93	2		
22		7 14 26·97	2	103 21 28·21	3
23		7 14 51·30	2	103 11 37·77	3
24		7 16 38·61	3	103 4 18·22	3
25		7 19 49·56	3	102 53 59·92	3
26		7 21 39·14	3	102 39 39·14	3
27		7 22 32·77	3	102 50 27·85	3
28		7 23 54·12	2	102 27 48·78	3

Cambridge Observatory, June 14, 1854.

J. CHALLIS.

The Comet of April (Comet II.) 1854. By Eyre B. Powell, Esq. F.R.A.S.

"On the evening of the 4th April last my attention was caught by a star-like object in the west, badly defined, and with a short, indistinct ray running upwards. On turning a $3\frac{1}{2}$-foot telescope upon the object, it was at once seen to be a comet. Time did not allow me to measure the right ascension and declination of the stranger that evening; but, on the 5th and succeeding nights, up to the 28th, with one or two breaks, I managed to secure observations more or less trustworthy. These are given in the following table; and they were taken with a 5-foot refracting telescope by Messrs. Simms, mounted equatoreally in a small octagonal room, erected for the reception of the instrument, within the grounds attached to the Government Native College. This room is exactly the counterpart of the one at the Madras Observatory, and was constructed under the superintendence of Captain Jacob, the Hon. East India Company's Astronomer. The object-glass of the telescope is 4 inches in diameter, and performs very satisfactorily; the magnifying power generally applied was about 55; but a higher power was used at first. On the 5th, 6th, and 7th April, I obtained the right ascension and declination of the comet directly from the readings of the equatoreal, the hour-circle of which measures seconds of time, and the declination-circle minutes of space. The errors of adjustment were found by comparing the instrumental right ascension and declination of a known star, having the same hour-angle and nearly the same declination as the comet, with the catalogued values of those quantities. On the remaining nights the position of the comet was ascertained by taking differential observations of it with one or more stars. At first the comparison stars were selected from the British Association Catalogue; but, towards the end of April, I was obliged to fall back upon Weisse, no large stars lying about the comet's path.

Observations (affected with Parallax and Aberration).

Day. 1853.	Madras Sid. Time. h m s	R.A. h m s	Decl. ° ′ ″
April 5	7 56 30	2 42 38	(+) 15 30 50
6	7 58 57·5	2 54 23·5	14 31 52
7	8 14 46	3 5 24	13 31 53
8	7 54 17	3 15 31	12 32 9
9	8 6 51	3 25 14	11 30 34
10	8 25 53	3 34 18	10 30 33
11	8 16 23	3 42 40	9 33 3
	8 49 36	3 42 51	9 31 39
12	8 13 43	3 50 29·5	8 36 42·5
13	8 53 0	3 58 4·4	7 40 54
15	8 34 10	4 11 23·3	5 58 59
16	8 49 40	4 17 34·6	5 10 12

Day. 1853.	Madras Sid. Time. h m s	R.A. h m s	Decl. ° ′ ″
April 18	9 4 55	4 28 48·6	(+) 3 39 38
19	9 6 24·5	4 33 58	2 57 36
22	9 28 38·7	4 47 55	1 1 37
23	9 18 25	4 52 10	(+) 0 26 19
24	9 29 7·5	4 56 14	(−) 0 7 33
26	9 33 59	5 3 46	1 10 52
27	9 47 46	5 7 19·6	1 40 51
28	9 45 24·4	5 10 45	2 9 32

Number of Comparisons or Direct Observations.

April 5. One direct observation.
6. Three direct observations.
7. Three direct observations.
8. One direct observation, and one comparison with B.A.C. 1087.
9. One direct observation, and two comparisons with 4 *Tauri*.
10. Three comparisons with B.A.C. 1174.
11. Four comparisons with Weisse iii. 814.
Three comparisons with B.A.C. 1180.
12. Two comparisons with B.A.C. 1295 and 1304.
13. Four comparisons with B.A.C. 1296.
15. Four comparisons with Taylor 1468 and 1499.
16. Six comparisons with Weisse iv. 361, 506, and 554.
18. Eight comparisons with Weisse iv. 608.
19. Seven comparisons with Weisse iv. 754.
22. Five comparisons with two small stars, which were again compared with *β Eridani*.
23. Three comparisons with Weisse iv. 1207.
24. Three comparisons with Weisse iv. 1346.
26. Three comparisons with Weisse v. 129.
27. Five comparisons with Weisse v. 169.
28. Two comparisons with Weisse v. 419.

"On the 5th the outline of the envelope was sharp; the length of the comet measured about 22′; and the nucleus was pretty bright, possibly equal to a star of the 3·4 magnitude.

"On the 6th I estimated the diameter of the nucleus at 8″; it was ragged, and equal to a star of the 4th magnitude. The tail was about 30′ long by measurement; also I fancied it not so broad as on the 5th.

"On the 7th the views I obtained were rather indistinct, in consequence of some clouds drifting across the western sky from south to north; but, with the exception of the nucleus appearing somewhat larger, the comet presented nearly the same aspect as on the 6th.

"On the 8th, owing partly to the moon, now on the increase and beginning to 'shine down' the faint light of the comet, the outline of the envelope was indiscernible. The nucleus was about as bright as a 6th magnitude star.

"On the 12th a small star was visible through the tail.

"On the 18th the comet would not bear any illumination, and the transits were taken on the lower edge of the micrometer comb. I trust that the care expended on the observations compensated in

a great degree for the imperfection naturally belonging to the mode of observing. The tail was longer, perhaps about 1° in length.

"On the 28th the comet was so excessively faint that the observations were taken with the greatest difficulty. In fact, it was no more than a small, whitish, cloudy patch; still there was something like a faint star as a nucleus: this was especially noted as the comet passed through the zero hole of the comb.

"Taking the observations of the 8th, 13th, 18th, and 27th, I proceeded to calculate the orbit.

"To obtain the perihelion distance and the time of perihelion passage, I used Pontécoulant's modification of La Place's method, given in the Appendix to tome ii. of the *Théorie Analytique du Système du Monde*. The following are the chief data and results; the notation corresponding to that of Pontécoulant:—

Epochs, Greenwich M.T.	Longitudes Observed.	Latitudes Observed.
	° ′ ″	° ′ ″
April 8·06067	49 50 56 (a°)	5 22 24 (b°) (−)
13·08769	59 0 3 (a)	12 33 19 (b) (−)
18·08229	66 1 16 (a')	17 55 12 (b') (−)

$$\frac{da}{dt} = +1{\cdot}63593$$

$$\frac{d^2a}{dt^2} = -4{\cdot}884596$$

$$\frac{db}{dt} = -1{\cdot}269075$$

$$\frac{d^2b}{dt^2} = +4{\cdot}17352$$

$$r^2 = 1{\cdot}00692 - 1{\cdot}62997 \cdot \varrho + 1{\cdot}0496 \cdot \varrho^2$$

$$\frac{1}{r} = {\cdot}496558 - 1{\cdot}8551335 \cdot \varrho + 2{\cdot}941991 \cdot \varrho^2$$

$$\frac{\delta\varrho}{dt} = + {\cdot}919686\,\varrho$$

"Assuming $r = {\cdot}65$, and $\varrho = {\cdot}99$, as suggested by trials,

$$\delta r = -{\cdot}00070196 \text{ and } \delta\varrho = -{\cdot}0008311$$

$$\therefore\ r = {\cdot}649298 \quad \text{and} \quad \varrho = {\cdot}9891689$$

"Using these values in approximating,

$$\delta r = +{\cdot}0000020 \text{ and } \delta\varrho = -{\cdot}0000007$$

Hence $r = {\cdot}6493$ and $\varrho = {\cdot}989168$

Also $$r \cdot \frac{dr}{dt} = +{\cdot}868954$$

"Therefore D, the perihelion distance = ·27176, and the time of perihelion passage = March 24·0028, v being 99° 22′ 34″.

"Then, by comparing the differences of the anomalies, afforded first by the parabolic theory *per se*, and secondly by it and the observations combined; and by tracing the effects upon those differences of variations in the above fundamental elements, I found, after about four approximations,

$\nu - u = -(1'-19'')$ for the error of anomaly between the 8th and 13th
and $\nu' - u' = +(4'-12'')$ — — — 8th and 27th

"The above errors might be almost eliminated by varying slightly the time of perihelion passage; but, without taking aberration and parallax into account, further calculation would appear to be labour thrown away.

"The corresponding elements are as follow:—

"*Elements.*

Time of Perihelion Passage, March 24·0501

Perihelion Distance	·27975
Longitude of Perihelion on Ecliptic	171 ° 21 ′
Longitude of Ascending Node	316 28
Inclination of Orbit	81 20

Motion retrograde.

"Hence it seems that the comet is not one whose elements have been already ascertained. There is, however, a considerable approach to agreement between some of its elements and the corresponding ones of a comet observed by Hevelius in 1677, the orbit of which was calculated by the celebrated Halley.

"Thus:—

	Motion.	Perihelion Distance.	Inclination. ° ′
Comet of 1677	Retrograde	·28059	79 3
Comet of 1854	Retrograde	·27975	81 20

"*Madras, May 16th*, 1854."

Elements of Proserpine, for the Opposition in 1854.

Computed by Mr. George Rümker, Observer at the Observatory, Durham, from the following four normal places, deduced from all the recorded observations at her last appearance:—

1853.	R.A. ° ′ ″	N.P.D. ° ′ ″	
May 20·5	205 10 21·8	99 49 5·5	from 27 observations.
June 3·0	204 3 20·1	99 47 55·9	— 14 —
27·0	204 12 20·1	100 13 41·2	— 9 —
July 6·0	206 23 23·7	101 28 7·9	— 14 —

M	349 16 41·9	1853, June 0·0	
π	235 24 35·7	1853·0	
λ	45 55 18·0		
i	3 35 45·0		
φ	4 55 51·1		
Log a	0·4236444		
Log μ	2·914541		

The following are the errors (computed—observed) in longitude and latitude:—

May 20·5	−0·4	+1·2
June 3·0	−0·1	−1·0
17·0	+0·3	−1·3
July 6·0	+0·6	+1·0

The errors in R.A. and N.P.D. of two observations at Washington, on the 23d and 24th of July, not included in those from which the elements are obtained, are

	δa	δp
July 23	+2·8	−0·2
24	−2·5	−1·7

The foregoing elements were accompanied with an ephemeris of the planet for the opposition of 1854, extending from July 15 to August 24. A few printed copies have been forwarded by Prof. Chevallier to the Apartments of the Society for distribution.

Notes on the Second Comet of 1854. By the Rev. T. W. Webb.

"As far as my opportunities of information have extended, this fine and conspicuous comet appears to have been noticed at Ross, in the county of Hereford, on the evening previous to its general recognition by English astronomers. About $7^h\ 30^m$ P.M. on the 28th of March, it was pointed out by an intelligent little boy of ten years old as "a stranger in the sky," and in consequence was seen by several persons before it sank into a bank of clouds in the N.W. horizon. From one of these, my friend, Mr. W. H. Purchas, I received immediate information of its appearance, and was thus enabled to watch its progress every night except two, from March 29th to April 5th, with a good achromatic by Tulley, of $5\frac{1}{2}$ feet focal length, and 3·7 inches aperture. In this and smaller glasses, as well as to the unassisted eye, it uniformly appeared in all its parts of a pale orange or fiery hue, and of an aspect which would have been deemed in former times terrible and portentous.

"During the earlier observations, with low powers, and comparatively near the horizon, the brilliant nucleus, of several seconds in diameter, was seen suddenly but not sharply defined, having

neither a stellar nor planetary appearance. With higher magnifiers its vivid light was diffused into a hazy mass of considerable brightness, not much condensed towards the centre. April 2d, it was thought smaller and more stellar with a power of 64. April 3d, this impression was confirmed; with 64 and 80 its aspect was almost that of a star; with higher powers up to 330, there was occasionally a suspicion of a strong luminous point in the centre of the diffused nebulous brightness; but this was seldom visible, and the air was too unsteady to admit of any certainty. In this respect it reminded me of the comet of July 1850, as well as of similar observations of Hooke and Schröter. With powers from 80 to 330, the light of the nucleus appeared condensed or accumulated, though but very slightly, on the side next the sun: this was also noticed on the following night, April 4th. April 5th, the nucleus was thought to be still becoming smaller and more definite. At the end of March it was as conspicuous to the naked eye as a 2d or 3d magnitude star, but it soon became less luminous.

"On March 29th, the nucleus was encompassed by a narrow border of faint yellow light, forming a hemispherical cap or envelope on the side turned to the sun, which seemed to have become less distinctly marked by April 3. From this envelope rose the tail, which was very narrow in proportion to its length, widened very gradually, and exhibited a decided curvature. Its convex side seemed a little better defined than the other on March 29th; this difference, however, on April 3d appeared to have become less sensible. Its length, notwithstanding a brilliant moonlight, was estimated on April 4th at least 5° by oblique vision with the naked eye. On March 30th, I began to perceive, with a comet eye-piece of M. Sécrétan's construction, that it ascended from the sides of the envelope in two apparent streams, very little denser, however, than the general mass, and soon becoming undistinguishable from it. April 4th, this indication of a hollow interior was perhaps still more evident, and the stream on the N. side seemed the more luminous, though on the following edge of the train as respects the orbital motion of the comet.

"On the first evening of my observations, as well as frequently afterwards, I thought there was, to the naked eye, aided only by a concave lens to correct near-sightedness, an appearance of blazing up, or flashing, issuing from the head into the denser portion of the tail; of this Mrs. Webb, whose eye is very sensitive to light, was on several occasions perfectly confident. These coruscations could not however be traced in the telescope.

"Mr. Purchas, at Ross, observed nearly the same phenomena with an achromatic of 4 inches aperture, as well as with the naked eye. At its first appearance on March 28th, he considered the nucleus as brilliant as *β Arietis*, with a tail extending about 3° in the strong twilight. He noticed the sharper definition of the convex side of the tail at first, as well as its subsequent decrease in this respect: its length was estimated by him on March 29th at 5°; and on March 30th he thought he could detect, without the telescope,

faint traces of two smaller and shorter tails, one on each side, diverging at a greater angle from the nucleus, and resembling some of the appearances figured by Schröter in 1811. This, however, was not confirmed by the telescope, nor did he see it on the following nights. He traced the same faint indications of a hollow structure in the train, especially on March 30th. On April 3d, and subsequently, they had become less evident. Coruscations appeared to him to be distinguishable both by the naked eye and with the telescope."

Photographic Pictures of the Moon taken with the Equatoreal of the Liverpool Observatory. By John Hartnup, Esq.

Mr. Hartnup exhibited ten collodion pictures of the moon, of 1·35 inches diameter, and ten enlarged copies, taken from the originals, which varied from 2 to $4\frac{1}{2}$ inches in diameter.

The photographs were all taken during the preceding month; and Mr. Hartnup stated that he only looked upon the progress now made as one step towards that degree of perfection which he had every reason to hope he should shortly arrive at.

In taking the pictures, the clock was so regulated that the telescope followed the moon in right ascension nearly; then, the hour-circle and the declination-circle being both clamped, the image of the moon was kept as nearly as possible in the same place, as seen in the finder, by means of the rods for giving slow motion to the instrument in right ascension and declination. The circles are each four feet diameter; the slow motion is given by means of tangent screws; and the instrument is so firmly mounted as to render the task of correcting for irregular motion of clock, change of moon's declination, and change arising from differences of refraction and parallax during the operation, comparatively easy.

The chemicals employed were those ordinarily used for taking a positive collodion picture on glass; and Mr. Hartnup acknowledged the great obligation that he was under to Messrs. Forrest, Berry, Edwards, M'Innes, and Towson, of the Liverpool Photographic Society, for the kind advice and assistance which he had from time to time received from them.

Of the photographs exhibited there were two (one taken before and one after the full), which, when placed in the stereoscope so as to be seen as one image, the appearance was that of a semi-transparent globe, the effect being perfectly stereoscopic. The enlarged pictures were shown on a screen by means of a magic lantern, kindly supplied for the occasion by Mr. Simms. Most of them were too large for the lens, so that only a portion of the picture could be seen at the same time on the screen, though it was eight feet diameter. There was, however, one photograph of a half moon, and one of a full moon, small enough for the screen; and these pictures were much admired. The President alluded to the gratifying progress of Mr. Hartnup's labours in connexion

with this interesting subject. At the same time he took occasion to point out the imperfect definition of the inequalities on the bright limb of the moon, and remarked that this was a defect of photographic representations, which placed them in strong contrast with the results of direct observation. To devise adequate means for remedying this defect was one of the principal objects to be aimed at in such experiments, and it was an essential element of success that there should be a contrivance for applying an equable and smooth motion to the surface destined to receive the photographic impression.

Mr. Burr states, in reference to his observation of the occultation of Mars inserted in the last *Monthly Notice*, that the estimated longitude of his observatory is $23^{s}\cdot8$ W.

Account of Observations of the Total Eclipse of the Sun of November 30, 1853, *made at Ocucaje, in Peru.* By M. Carlos Moesta.*

(*Communicated by Admiral W. H. Smyth.*)

The author of this account, who is Director of the National Observatory at Santiago, Chili, was appointed by the Chilian Government to proceed to the neighbouring republic of Peru for the purpose of making observations of the total eclipse of the sun which was to occur on the 30th of November, 1853.

By a graphic delineation, based upon the results given in the *Nautical Almanac*, M. Moesta found that the central line of the totality of the eclipse would enter the coast of Peru from the Pacific at a point situated in long. 76° 2′ west, and lat. 14° 27′ south. Pisco was the nearest town to this part of the coast, but M. Moesta resolved to observe the phenomenon at some convenient place in the interior, elevated as much as possible above the level of the sea.

The various instruments required for observing the phenomenon were obtained from the Observatory of Santiago. Among these was an achromatic refractor of $4\frac{1}{2}$ inches aperture and $5\frac{1}{3}$ feet focal length, by Frauenhofer, furnished with a reticulated micrometer adapted for measuring differences of right ascension and declination, and also angles of position. Another telescope, a refractor, by Dollond, of $2\frac{3}{4}$ in. aperture and $3\frac{1}{2}$ feet focal length, was used by M. Lira, the assistant of the Observatory, who accompanied the author in his expedition. Besides these two instruments, the Observatory furnished for the observation of the phenomenon a reflecting circle by Ertel, divided to every 10″, fitted with two

* This Report was originally drawn up by M. Moesta and presented to the Minister of Public Instruction of the Chilian Republic.

verniers, a sextant by Pistor, a good chronometer by Parkinson and Frodsham, a barometer, and various thermometers.

The author embarked at Valparaiso on the 1st of November, and arrived at Pisco on the 9th of the same month. By means of ten altitudes of the sun, taken on the 11th, he found that this town was situate $22^m\ 12^s{\cdot}78$ west of the meridian of the Observatory of Santiago. On the 13th of November, while still residing at Pisco, the author carefully examined the sun's disk with the view of ascertaining whether any spots or faculæ were visible which might serve to throw light upon some of the phenomena of the approaching eclipse. Two spots were visible near the eastern margin of the disc, one of which consisted of a group of smaller spots united together like the links of a chain. These spots continued to be seen till the 20th of the month, but no other spot had appeared on the eastern margin of the disc during the interval of their being visible. It is to be remarked, also, that these could not have remained on the disc till the time of the eclipse.

Finding that Pisco was situate in an unfavourable locality for observing the eclipse, the author removed from that town on the 23d, and arrived on the 25th at Ica, which from an old Spanish map he concluded to lie in the central line of the shadow. He found, however, from observations made on the evening of his arrival that the latitude of Ica was in reality only 14° 5′, and that it was situate $1^m\ 56^s$ to the east of Pisco. It was accordingly necessary to advance about eighteen miles farther towards the south. On the evening of the 27th M. Moesta therefore pursued his journey in that direction, bearing a little towards the east, and on the following morning arrived at Ocucaje, a place which from actual observation he found to be situate in lat. 14° 21′, and to be 20^s east of the meridian of Ica. He consequently found himself within about half a mile of the central line of the shadow.

M. Moesta selected for the place of observation an eminence, which, from barometrical measurement, he found to be 880 feet above the level of the sea. On the 28th, he observed a series of corresponding altitudes of *α Eridani* and *α Argus*, from which the latitude appeared to be 14° 21′ 21″. On the morning of the 30th, the sky was everywhere overcast, but about 8 o'clock the sun made his appearance, and the heavens gradually cleared up, promising to afford a full view of the eclipse to the numerous spectators who had congregated to this sequestered spot for the purpose of witnessing so unusual a phenomenon.

All the necessary preparations were now made for observing the eclipse. The error of the chronometer was determined by several altitudes of the sun. The telescopes were placed in appropriate positions. It was arranged that M. Lira should note the variations of the barometer and thermometer; Mr. Clark, an intelligent traveller from North America, undertook to observe all the phenomena which might be visible to the naked eye.

The following account of the appearances observed during the eclipse is thus given by the author:—

"A few hours before the commencement of the eclipse, I examined the disk of the sun with the Frauenhofer refractor, in order to ascertain whether any spots were visible, and to fix their position if there should be any; but all my efforts were unsuccessful. There was not the slightest spot or interruption of the surface, and the disc appeared perfectly round. I make particular mention of this circumstance on account of its importance as regards the red protuberance seen during the total obscuration, since it has been generally supposed that some relation exists between these phenomena and the solar spots. I shall presently have occasion to speak of this protuberance.

"Two minutes before the commencement of the eclipse, I directed my telescope to the point of the margin of the sun where it was expected that the moon would make her exterior contact. At $2^h\ 13^m\ 25^s{\cdot}5$ mean time of the place of observation, I perceived the first contact of the two discs. No extraneous phenomenon appeared to accompany this occurrence. The moon gradually advanced over the sun's disc; her projection upon the latter tracing out a dark and well-defined arc. On the southern margin of the moon, many elevations were visible, but none of them was sufficiently prominent to admit of ascertaining its height. At $2^h\ 52^m$ the whole western part of the horizon had cleared up. At $3^h\ 8^m$ the small clouds in the vicinity of the sun had disappeared, but the horizon towards the north and east still remained covered with dense clouds. At this time the wind from the south blew with less violence, and the cold began to make itself sensible. The landscape and surrounding objects had changed their colour, and everything seemed to announce the approach of the decisive moment. At $3^h\ 32^m$, while fixing my attention on the eastern part of the sun's disc, where the uncovered space gradually became narrower and narrower, while the two limbs of the sun and moon were perfectly distinguishable, a nebulous substance of a reddish colour, apparently in a state of ebullition, all of a sudden filled up the space between the two limbs. This substance appeared to be traversed by a vein of colour more obscure. It continued to accumulate as the space between the two limbs became narrower, as if it had been forced up between them. Finally, it acquired a ruddy colour of so dark a tinge that it became difficult to distinguish clearly the two limbs. The contact of the latter took place at $3^h\ 32^m\ 15^s{\cdot}5$; at the same instant the last ray of the sun and the last trace of the nebulous substance vanished. From this moment the eclipse presented a very imposing aspect, nature appearing in all her majesty.

"The moon, whose diameter was then $82''$ greater than that of the sun, resembled a round and perfectly black circle, and to the naked eye appeared extremely well defined at its circumference. Around it was visible a luminous ring called a *corona of light*, whose soft lustre had a colour between white and azure, like the light of a gas-burner. The light of the ring was as bright, at the part where it touched the moon, as the light of the full moon in a

clear night at Santiago; but its intensity diminished rapidly, insomuch that, at a distance equal to a fourth part of the lunar diameter, it confounded itself with the pale light of the sky. Conceiving for a moment a vertical circle passing through the centre of the moon, the ring will then divide itself into two parts; viz. one situate to the south, and the other to the north, of the circle in question. Almost all the northern part of the ring was uniform; the opposite part, however, was composed of numerous rays which appeared to issue from the ring, and which all seemed to have the same length with the exception of two very large rays. With respect to the latter, one of them was perceived to shoot upwards, inclining at an angle of 20° to the south of the vertical, and according to estimation extended to a distance equal to the diameter of the moon. The other issued downwards, not diametrically opposite to the former, but inclined about 10° to the north of the vertical, and was rather shorter. The aspect of these two rays very much resembled the tail of a comet, which was narrower at the extremity than at the nucleus, and which was very radiant in all its structure, since they manifestly did not emit a uniform light, but were composed of numerous small rays.

"Immediately after the sun was entirely concealed from view there was visible, in the northern part of the corona of light, a rose-coloured protuberance resembling a cloud, which seemed to issue from the margin of the moon. Its form, which was longitudinal, and also its initial position, are indicated in the sketch.* The colour of this protuberance was not uniform, but was evidently darker in some parts than it was in others. The form continued the same, but as regards its position, I thought I could be certain that it moved towards the ray which shot upwards from the *corona of light*. Gradually its colour became fainter, and after the lapse of about $1^m\ 20^s$ from its first appearance, the whole of the protuberance had vanished. With a position-micrometer I measured its height above the margin of the moon, which I found to be 1′ 8″.

"On the same side of the *corona* there were seen during the whole time of the obscuration two dark spots, not round, but rather of a square form, and attached to the margin of the moon. It seemed as if there had been an interruption of the ring in that part, or rather two apertures through which the dark ground of the heavens was visible. This could be no optical illusion, since many persons were present who could discern the spots referred to perfectly well with the naked eye. Their height above the margin of the moon was estimated at 1′. * * * * *

"The corona disappeared instantaneously with the reappearance of the first ray of the sun, since neither before nor after the total obscuration was the slightest vestige of it to be seen: there was also a similarly sudden change in the brightness of the atmosphere. So long as even the smallest part of the solar disc remained uncovered, it was possible to perceive surrounding objects almost as well as on an

* The drawing here referred to has not been communicated to the Society.

ordinary day, except that every object assumed a peculiarly pale hue. At the moment of the sun being wholly concealed from view, there suddenly came on a kind of darkness very different from that which usually characterises the evening twilight. Towards the horizon, and over the whole north and east where the sky continued cloudy, the darkness of the atmosphere resembled night; while, on the other hand, in the neighbourhood of the moon it was much less intense. The degree of obscurity may be judged of from the following observations:—the planets *Venus* and *Jupiter* were seen in all their brilliancy; *Antares*, which was within 5° of the sun, and was consequently in the immediate vicinity of the corona, was distinctly visible to the naked eye; and as that star is between the first and second magnitude, it might be generally asserted that the degree of obscurity was such as to allow of stars of the second magnitude being seen. There was not sufficient time to look for other stars.

"I had beforehand placed the first page of the *Nautical Almanac* near the telescope, and during the total obscuration I could read it very distinctly at the distance of two or three feet. M. Lira could read the scale of the thermometer without the least difficulty.

"The total obscuration lasted $2^m\ 59^s$, since it ended at $3^h\ 35^m\ 14^s{\cdot}6$.* *Jupiter* remained visible some minutes afterwards, and *Venus* shone brilliantly till $4^h\ 3^m$, that is to say, twenty-eight minutes after the total obscuration." * * * *

The author remarks, that the researches on the physical constitution of the sun seem to indicate the existence of a dark body enveloped in two atmospheres, the interior being non-luminous and somewhat resembling our own atmosphere, while the exterior one, or photosphere, is the exclusive source whence emanates the solar light. Referring, then, to the observations of the total eclipses of the sun of 1842 and 1851, he thus proceeds:—

"All these observations render it probable that the sun has a third atmosphere, imperfectly diaphanous, enveloping the photosphere, and that through the apertures of the spots there issue gaseous exhalations, which, rising to the third atmosphere, form there the coloured clouds, constituting the observed protuberances.

"Thus it arises that our knowledge relative to the physical constitution of the sun depends chiefly upon exact observations of total eclipses of that body, and it is, consequently, of importance that we take advantage of such eclipses as successively occur, in order to improve and amplify the little that we know upon that subject.

"Turning now to the observations made in Peru, I have to remark, that there was visible during the total obscuration a red protuberance, although a corresponding spot on the sun's disc was

* The author states in another part of his account that the eclipse terminated at $4^h\ 43^m\ 41^s{\cdot}8$.

not afterwards perceived. *It hence results that the supposed connexion between the spots visible on the solar disc and the red protuberances is destitute of foundation*, and that the so-called protuberances are in reality gaseous exhalations, which becoming visible in the third atmosphere, must have issued, during the eclipse of the 30th of November, through very small apertures of the photosphere. Such an hypothesis is by no means improbable, since there is always discernible on the solar disc, by the aid of powerful telescopes, a net-work of pores subject to perpetual change.

"The short duration of the protuberance, joined to its change of colour and its apparent motion, are circumstances which seem to indicate the imperfect transparency of the third atmosphere, as well as whirling currents of wind which occur in it, analogous to those which prevail in the terrestrial atmosphere.

"The corona of light has been seen during several former eclipses, but, as far as I am aware, in no instance with such brilliancy, or with such a peculiar form, as on the present occasion. The radiant light of the corona, throughout all its southern side, strongly resembled the aurora borealis; but the two large emanations of light, above referred to, exhibiting the radiation upon a much larger scale. The contrast between this part of the corona and the northern part, whence a soft light, unaccompanied by rays, appeared to emanate, imparted a singular aspect to the phenomenon. I am of opinion, that the latter may be satisfactorily explained by supposing the activity of the photosphere, not to be equal in all its parts, but that by some cause it is more violently agitated in certain places than in others. It is not so easy to give a satisfactory explanation of the two dark spots above mentioned. I refrain from making any remarks upon the extreme transparency of the terrestrial atmosphere in this part of Peru. The foregoing observations sufficiently attest this fact."

Meteorological and other Observations made during the Eclipse.

In order to ascertain with greater precision the influence of the eclipse upon the thermometer, M. Moesta caused observations with two thermometers to be made, not only on the day of the eclipse, but also on the preceding and following days. One of these thermometers was placed in the shade; the other, which had a blackened bulb, was exposed to the direct influence of the sun's rays. The author then gives the details of the observations on each day. It appears from them, that on the 30th of November, during the time comprised between the commencement of the eclipse and the total obscuration, the thermometer exposed to the sun, sank $22^{\circ}{\cdot}5$ Fahrenheit, after which it continued to rise till $4^{h}\ 15^{m}$, attaining, in this interval, an elevation of $3^{\circ}{\cdot}8$. On the other hand, the same thermometer was observed on the 29th to descend $5^{\circ}{\cdot}2$ during

the former of these two intervals, and *continued to descend* 6°·7 during the latter interval. On the 1st of December the descent, during the former interval, was 9°·7.

"A change of 14°·4 in the temperature during little more than an hour ought certainly," says the author, "to make itself sensible; and the truth is, that during the time of the total obscuration I was shivering with cold." This sudden fall of the temperature was accompanied by an interesting phenomenon, which manifested itself under very striking circumstances. At the entrance of the shadow on the coast of Peru, the air, usually so hot from the sandy plain upon which it rests, suddenly condensed from the effect of the eclipse, and this caused a current of cool air to flow from the ocean for the purpose of restoring the equilibrium of the atmosphere. There ought from this cause to have been a wind from the west; however, as a cool breeze from the south had continued to blow during the whole day, there was a violent meeting together of two distinct currents, and the consequence was, that during the total obscuration a succession of whirlwinds occurred which in some instances raised the sand to a height of 200 feet.

Another phenomenon strongly attracted the attention of the author,—a species of cloud of a pale azure colour, mingled with purple. To the south of his position, at the distance of half a league, a table-mountain was situate, from which this cloud appeared to ascend in columns at three o'clock in the afternoon. It continued to augment till the time of the total obscuration, when it presented a very dense and black aspect. It then gradually grew less, and, finally, disappeared a little before the termination of the eclipse.

M. Moesta also remarked that, during the total obscuration, the leaves of the carob-tree closed up as if it were night, and resumed their natural position shortly after the reappearance of the sun. In the case of the leaves of the cotton-tree, Mr. Clark noticed a similar change. The latter also remarked that, during the total obscuration, all sorts of insects disappeared, that the fowls grouped together, and that other species of poultry continued flying about very near the ground. "Great," says the author, "was the impression which the occurrence of so many rare phenomena during the total obscuration produced on the minds of the spectators, who all saluted with enthusiasm the first rays of the sun on the occasion of his reappearance."

Remarks on the Early History of the Researches of Astronomers relative to the Spheroidal Figure of the Earth. By the Editor.

The establishment of the spheroidal figure of the earth must ever be regarded as one of the noblest results of modern scientific inquiry. It must be acknowledged that a good deal of misconception has hitherto prevailed respecting the original researches connected with this important problem of mechanical astronomy. It has been generally admitted by writers on astronomy that Newton and Huyghens first demonstrated by strict mathematical reasoning that, under certain conditions as regards the mutual action of its constituent particles, the figure of the earth would be spheroidal, the equatoreal being to the polar axis in a certain assignable ratio, which each of these geometers computed according to his peculiar view of the physical conditions of the problem. In this inquiry, however, the priority in respect to time has been, in some instances, awarded to the labours of Huyghens, upon the evidence of a passage contained in his "Dissertation on the Cause of Gravity." Delambre, who was at first led to give currency to this view of the subject, had, subsequently, the merit of proving it to be inconsistent with the records of history. From an examination of the early registers of the Royal Academy of Paris and a careful interpretation of Huyghens' own words, as expressed in the work above alluded to, he demonstrated beyond doubt that the original ideas of the latter on the spheroidal figure of the earth were of a considerably later date than that usually assigned to them, and that, in his solution of the problem for determining the ratio of the equatoreal and polar axes of the earth, the Dutch geometer had been preceded by Newton, whose method he had, in fact, merely applied to his own theory of gravity. It may be remarked, however, that both these points admit of being satisfactorily established by a reference to Huyghens' work alone, independently of any other source of evidence.

But, in connexion with this subject, there still remains the interesting point to be examined, whether Newton was originally led to conjecture the spheroidal figure of the earth by reflecting on the consequences of the theory of gravitation, or whether he was indebted to some extraneous source for the suggestion of so novel an idea. Humboldt, in a recently published work (*Kosmos*, Band I. s. 172), seems disposed to think that Newton received the first hint on this subject from Cassini's observations of the spheroidal figure of *Jupiter*. This might seem at first sight to be impossible, since it is generally understood that Cassini first announced to the world his discovery of the spheroidal figure of *Jupiter*, in a paper communicated to the Academy of Sciences in the year 1691, that is to say, four years subsequent to the publication of the *Principia*. Humboldt, however, remarks that Cassini's earliest observations of the spheroidal figure of *Jupiter* ascend to an epoch which is at least twenty-one years anterior to the publication of Newton's immortal work. In support of this

statement he refers to a passage in the third volume of Lalande's *Astronomy*, wherein the author asserts that he saw in Maraldi's possession several printed sheets of a fragmentary treatise by Cassini on the spots of the planets, from which it appeared that, at least as early as the year 1666, the Italian astronomer had ascertained the spheroidal figure of *Jupiter*. The illustrious philosopher just referred to has suggested that Newton may have become acquainted with these observations of Cassini by personal intercourse with the scientific men of his day, and thus have been led by analogy to his remarkable conjecture relative to the figure of the earth.*

In the hope of obtaining materials which might throw some light upon this interesting subject, I recently undertook an examination of various historical records, the results of which I have embodied in the following notes. It affords me great pleasure to have this opportunity of expressing my sincere thanks to the President and Council of the Royal Society, who very kindly accorded to me the privilege of examining the early MSS. of the Society while engaged in this inquiry.

Statement of Facts connected with the Early History of the Researches of Astronomers on the Spheroidal Figure of the Planets.

1664, Dec. 14. At a meeting of the Royal Society, Hooke remarked that as everything in nature is liable to change, so the force of gravity might probably vary at the earth's surface. He added that, as it seemed to have an affinity to the magnetic virtue, it might, as in the case of that force, *be more intense at the poles than at the equator.*—(*Journal Book of the Royal Society*, vol. i.)

1666. According to Lalande, the spheroidal figure of *Jupiter* was remarked by Cassini previous to this year.—(*Astronomie*, 3^{ieme} edition, tom. iii. p. 335.)

1667, April 3. It is recorded in the Registers of the Royal Academy of Paris, corresponding to this date, that *the experiment with the pendulum is conformable to the theory of Copernicus.*—(Delambre, *Hist. Ast. Mod.*, tome i., Addit. p. lxiii.)

1671. The *Mésure de la Terre* of Picard was published in this year. Alluding to the standards of measure of different lengths which are derivable from observations of the rate of oscillation of the pendulum, the author thus remarks:—"Ces mésures uni-

* M. Humboldt evidently attaches considerable importance to this point in the history of the inquiry respecting the figure of the earth, for he alludes to it in the same work on two subsequent occasions. In support of his views, he cites a passage of Brewster's *Life of Newton*, wherein the author states, that Cassini's observations of the oblate figure of *Jupiter* had *probably* suggested to Newton the idea of the earth having a similar figure. I am gratified to state, that the distinguished philosopher to whose authority M. Humboldt refers has expressed himself perfectly satisfied with the conclusions at which I have arrived in this communication. I am inclined to think that if the evidence here presented had been accessible to M. Humboldt, he would also have been induced to modify his opinions on the subject.

verselles supposent que la différence des lieux ne cause aucune variation sensible aux pendules. Il est vrai qu'on a fait à Londres à Lyon et à Boulogne en Italie quelques expériences, d'où il semble que l'on pourrait conclure que les pendules doivent être plus courts à mésure que l'on avance vers l'équateur: *conformement à la conjecture qui avait été deja proposée dans l'assemblée, que, supposé le mouvement de la terre, les poids devraient déscendre avec moins de force sous l'équateur que sous les poles.*"—(*Mésure de la Terre*, p. 5.)

1673, April 13. In the *Histoire Céleste* of Lemonnier, consisting of a collection of extracts from the Registers of the Royal Observatory of Paris, which was first published in the year 1741, there is to be found the following observation by Picard, corresponding to this date:—"Je commençai à m'aperçevoir que le disque de *Jupiter* était *un peu ovale*, et que le plus grande diamètre est toujours suivant les bandes."—(*Hist. Cél.* p. 28.)

1678, Feb. 28. At a meeting of the Royal Society, a discussion arose respecting a remark made by M. Gallet, in an account of the transit of *Mercury*, observed by him at Avignon, on the 7th of November, 1677. M. Gallet stated that when the planet was projected on the sun's disc, it appeared to be of an oval figure, the longest diameter being parallel to the equinoctial.* Mr. Henshaw suggested that the appearance might arise from the refraction of an atmosphere about the planet. Mr. Hooke conceived that the body itself of *Mercury* might be of such a figure; and that it might proceed from the velocity of its whirling round upon its axis, he supposing that the axis of its vertiginous or diurnal motion lies north and south, or at right angles with the seeming motion of its parallel to the ;† that a very swift vertiginous motion on that axis made the body of *Mercury* somewhat of the shape of a turnip, or of a solid made by an ellipsis turned round upon its shorter diameter.—(*Journal Book of the Royal Society*, Vol. vi. p. 55.)

March 7. At the meeting held this day, the remark of M. Gallet respecting the oval figure of *Mercury* was further discussed. Hooke's hypothesis was objected to on the ground that, although such an oval figure would be caused by the whirling round of a fluid body, yet it was probable that the body of *Mercury* is solid; and, consequently, the whirling could have no effect upon its figure. To which Mr. Hooke replied, that although it might now be a solid body, yet that at the beginning it might have been fluid enough to receive that shape; and that, though this supposition should not be granted, it would be probable enough that it would readily run into that shape, and make the same appearance; and that it is not

* This is manifestly the spurious phenomenon (termed the *gutta nigra*) which has been observed in modern times on the occasion of transits of the inferior planets. See Baily's paper "On the Annular Eclipse of the Sun of 1836."—*Mem. Ast. Soc.* vol. x.

† This *hiatus* appears in the original MS.: the reader will have no difficulty in supplying the omission.

improbable but that the water here about the earth might do it in some measure by the influence of the diurnal motion, which, compounded with that of the moon, he conceived to be the cause of the tides.*—(*Journal Book*, vol. vi. p. 60.)

1679. Richer's work, containing the account of his observations with the pendulum at Cayenne, was published in this year. It is stated by the author, that the pendulum was found to oscillate more slowly at Cayenne than at Paris.

1686, July 14. In a letter to Halley respecting the claims of Hooke to the discoveries announced in the *Principia*, Newton thus writes:—"There was another thing in Mr. Hooke's letters which he will think I had from him. He told me that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for in that very paper, which I told you was writ sometime above fifteen years ago, and, to the best of my memory, was writ eighteen or nineteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet, to do this business right, is a thing of far greater difficulty than I was aware of.—(*Guard Book of the Royal Society*, N. 1.)

1686, Sept. 3. Newton thus writes to Flamsteed:—"There is another thing said to be observed of *Jupiter* by Cassini long since; namely, that his diameter from pole to pole is shorter than from east to west. If this were certain, it would conduce much to the stating of the reason of the precession of the equinoxes."—(*Biog. Brit.* art. Newton, p. 3229.)

1687, Feb. 2–9. At the meetings of the Royal Society held on these days, Hooke contended that the figure of the earth is that of a spheroid, the shortest axis of which passes through the poles; and he cites, in support of this opinion, the oval figures of the Sun, *Mercury*, and *Jupiter*, as observed by Scheiner, Gallet, and Cassini, respectively.—(*Journal Book*, vol. viii. pp. 127–129.)

1687, April 6. Book iii. of the *Principia* was presented to the Royal Society. In Prop. xviii. there appears the following statement:—"Sic Jovis diameter (consentientibus observationibus Cassinii et Flamstedii) brevior deprehenditur inter polos quam ab oriente in occidentem."

1690. Huyghens' treatise, entitled, *Traité sur la Cause de la Pesanteur*, was first published.† The author states in this work

* These remarkable *aperçus* of Hooke's on the figure of the earth are also to be found in Birch's *History of the Royal Society*. but they seem to have wholly escaped the notice of scientific writers. It may be mentioned, that the words of the original MS. are closely adhered to in the passages here cited.

† This work was originally published in French at Leyden, in the year 1690, while Huyghens was still alive. A Latin edition was afterwards published at Amsterdam in 1728, along with several posthumous works of the author.

that he was originally led to conclude that the figure of the earth is spheroidal by reflecting on the variable rate of the oscillations of the pendulum in different latitudes, as indicated by the observations of Richer at Cayenne. He also mentions, that shortly after he arrived at this conclusion, he was led to investigate the actual ellipticity of the earth upon his own hypothesis of gravity by the perusal of Newton's researches on the same subject, contained in the *Principia*.

Remarks on the foregoing Statement of Facts.

It will be seen that there exists no necessity for appealing to the authority of Lalande in regard to the question of Cassini having observed the spheroidal figure of *Jupiter* previous to the publication of the *Principia*, since Newton expressly alludes to the observations of the Italian astronomer, both in his letter to Flamsteed, above cited, and also in the original edition of the *Principia*. It is manifest, however, that Cassini did not place much reliance upon his early observations of the ellipticity of *Jupiter*, since we find him asserting, as late as the year 1690, that the planet appeared quite round (*Anc. Mem. Acad. des Sciences*, tom. ii. p. 108).

It is worthy of remark that, although Flamsteed is cited by Newton, in the first edition of the *Principia*, as having ascertained by observation that the figure of *Jupiter* is spheroidal, this positive statement is suppressed in the second and third editions. Instead of the words, "consentientibus observationibus Cassinii et Flamstedii," which appear in the original edition (Book iii. Prop. xviii.), the words, "consentientibus astronomorum observationibus," are used in the second and third editions. It might seem, at first sight, as if this alteration had no special reference to Flamsteed, since the name of Cassini is also omitted. We find, however, in the following proposition of both editions, an express mention of the observations of Cassini, but not of those of Flamsteed; whereas, in the original edition, the same proposition contains no allusion to the observations of either of these astronomers. I shall not presume to suggest what may have been the motive which induced Newton thus to suppress the name of Flamsteed in connexion with so interesting a fact as the independent observation of the spheroidal figure of *Jupiter*. I may state, however, that I have carefully examined the *Historia Cælestis*, and also Baily's *Life of Flamsteed*, but I have not found in them any suspicion of the planet being otherwise than perfectly round. It appears to me that, if Flamsteed had reposed any confidence in his observations, I should have found them recorded in one of the works just cited.

It appears that as early as the year 1667 or 1668, Newton was led to consider the diminution of gravity at the equator arising from the diurnal motion of the earth. As he was then endeavouring to explain the mechanics of the solar system by the principle of gravitation, it can hardly be doubted that the spheroidal figure of

the earth, which is a direct consequence of this diminution, had already presented itself to his mind. It would seem from his letter to Flamsteed, that he had only obtained some vague information respecting Cassini's observation of the spheroidal figure of *Jupiter*, and that he regarded such an observation as affording a valuable confirmation of his own abstract researches on the spheroidal figure of the earth upon which the phenomenon of the precession of the equinoxes (alluded to by him) essentially depends. It is certain, however, from the words used by him in his letter to Halley, "But yet to do this business right is a thing of far greater difficulty than I was aware of,"—that he had not at this time devised the method for computing the ratio of the equatoreal and polar axes which he has given in the *Principia*. The difficulty to which he refers manifestly arose from the complication of the effects of the centrifugal force at the equator with those due to the variation in the intensity of gravity occasioned by the greater distance from the centre of the earth of the particles constituting the redundant matter at the equator.

That Cassini's early observations of the spheroidal figure of *Jupiter* were regarded by Newton as valuable, inasmuch as they served, not to suggest, but to confirm, his own theoretical researches, will further appear evident from the following considerations:—

In 1678, when the question of the spheroidal figure of the earth was discussed at several successive meetings of the *Royal Society*, no allusion was made by Hooke, or any other Fellow of the Society, to Cassini's discovery of the spheroidal figure of *Jupiter*. As this fact would have served to throw much valuable light upon the subject of discussion, we may fairly conclude that it was altogether unknown to any of the individuals who attended these meetings. But if Hooke and his scientific friends in London had not yet obtained any account of Cassini's observations, it is in the highest degree improbable that Newton — who was living in comparative retirement at Cambridge, and whose intercourse with men of science was at this time very limited — should have been better informed on the subject.

It seems probable that the presentation of the first two books of the *Principia* to the *Royal Society* in the spring of 1686, and Halley's active dissemination of a knowledge of the important labours of Newton on the System of the World, may have given rise to much discussion both in England and on the Continent, and that out of this discussion, Cassini's discovery of the spheroidal figure of *Jupiter* (or rather his suspicion of that fact, for it was nothing more) first became known in this country. This view of the subject will appear still more credible when it is borne in mind, that at one of the meetings of the *Royal Society*, held in the spring of 1686, we now find Hooke, for the first time, alluding to the observations of Cassini.

That an important scientific fact closely connected with Newton's researches, might have remained unknown to that geometer

many years subsequent to its discovery, is strikingly illustrated by the circumstance that, at the time of the publication of the second edition of the *Principia*, in 1713, he appears to have been unacquainted with Cassini's comparatively recent communication to the Academy of Sciences, in 1691, respecting the spheroidal figure of *Jupiter;* for, if he had been aware of that announcement, he would assuredly have referred to it in the new edition; whereas, we find merely a vague allusion to the early observations of Cassini. It is in the third edition, which was published in 1726, that Newton for the first time alludes to Cassini's communication to the Academy of Sciences in 1691, the vaguer statement which appeared in the second edition being now altogether suppressed.

It can hardly be doubted, when we take into consideration these various circumstances, that, when Newton first became acquainted with Cassini's early observations of the spheroidal figure of *Jupiter*, he regarded them as valuable merely on account of their affording a confirmation of his own theoretical researches on the spheroidal figure of the earth and the precession of the equinoxes.

About the same time that Newton was led to suspect the variation of gravity at the surface of the earth, or at any rate prior to the year 1671, a suspicion of the same fact was announced at one of the meetings of the Academy of Sciences of Paris. It is worthy of remark, however, that, although Delambre carefully examined the early registers of the Academy, he was unable to discover in them the slightest allusion to the spheroidal figure of the earth.

The earliest distinct announcement of the probability of the earth being somewhat flattened at the poles, as a consequence of its diurnal rotation, is, undoubtedly, due to Hooke. The hints on the subject thrown out by that distinguished philosopher at several successive meetings of the Royal Society in the spring of 1678, afford abundant proofs of the sagacity of their author; but, unfortunately, as in the case of his other speculations on the principle of gravity, he failed to establish his conclusions by a process of rigorous investigation.

It is difficult to ascertain the precise time at which Huyghens was led to suspect that the figure of the earth must be spheroidal. It would appear from the preface to his essay, *On the Cause of Gravity*, that the part of the work which treats of this subject was written after his departure from Paris, and, therefore, subsequently to 1681; and as we further gather from another statement made by him in the body of the same work, that he had been led to suspect the spheroidal figure of the earth *previous* to his perusal of the *Principia*, we may infer that his original speculations on the subject were suggested to his mind on some occasion during the interval comprised between the years 1681 and 1687. His subsequent computation of the terrestrial ellipticity, according to his own ideas of gravity, was suggested by the method given by Newton in the *Principia*.

It may be remarked that the spheroidal figure of the earth was originally suggested to Hooke by the announcement of an ana-

logous fact (which, however, turned out to be unfounded), as characterising one of the other bodies of the planetary system. Huyghens, on the other hand, was led to a similar conclusion, by reflecting upon the variation of gravity at the earth's surface, as indicated by the observations with the pendulum. It is highly probable that Newton's views on the same subject were suggested by purely mechanical considerations.

The following conclusions may be considered as fairly deducible from the facts above stated :—

1. The earliest authentic announcement of the spheroidal figure of *Jupiter* is contained in an observation of the planet by Picard in the year 1673. There are good reasons for supposing that about the same time, or even previously, Cassini was led by his observations of the planet to suspect the same fact; but the Italian astronomer does not seem to have entertained a firm conviction of its existence till about the year 1691. It would appear from the evidence of Newton, that Flamsteed was also one of the early observers of the spheroidal figure of *Jupiter:* this statement, however, is not borne out by an actual examination of the recorded observations of the English astronomer.

2. In the year 1667, or 1668, Newton seems to have been led by his speculations on gravity to consider the effects of the centrifugal force in diminishing the weight of bodies at the equator. It is highly probable that the spheroidal figure of the earth had occurred to his mind on this occasion; but we have no reason to suppose that he was acquainted, at so early a period of his researches, with the method subsequently given by him in the *Principia* for determining the ratio of the equatoreal and polar axes.

3. About the time when Newton first turned his attention to the effects of the centrifugal force at the equator, occasioned by the diurnal rotation of the earth, the probability of a diminution of gravity from the poles to the equator, arising from the same cause, was suggested at one of the meetings of the Royal Academy of Sciences of Paris. It does not appear, however, that any inference was deduced from this fact relative to the spheroidal figure of the earth.

4. In the year 1678, Hooke suggested that the figure of the earth might be spheroidal in consequence of its diurnal motion, but neither on that nor on any future occasion did he assign a method for determining the ratio of the equatoreal and polar axes.

5. Some time during the period comprised between the years 1681 and 1687, Huyghens arrived at the conclusion that the figure of the earth must be spheroidal, but his subsequent determination of the ellipticity, according to his own hypothesis of gravity, was founded on the method given by Newton in the *Principia.*

6. There exist no grounds for supposing that Cassini's observations of the spheroidal figure of *Jupiter* were known in England previous to the year 1686.

New Planet

Discovered July 22 at $11^h\ 45^m$ mean time.

1854.	Regent's Pk. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″
July 22	11 56 55	21 9 50·69	106 20 26·:
..	13 9 29	21 9 48·42	20 45·5
..	13 40 47	21 9 47·72	20 50·2
23	10 46 52	21 9 1·29	106 23 13·7

The Planet shines as a star of 10.9 magnitude.

J. R. HIND.

Mr. Bishop's Observatory,
Regent's Park, July 24.

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A Supplementary Number will shortly be issued, which will contain a few additional Papers, besides the Index and other matters relating to the completion of Vol. XIV. of the Monthly Notices.

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